now appears that the three rivers flow on the surface of the 12,000 feet high Tibet plateau, and are separated—not by fan-like radiating mountain ranges, but by ranges of mountains rising some 3000 feet above the plateau and all running parallel to each other, N.W. to S.E. In its western portion, the high plateau, deprived as it is of the rains of the monsoon region, is a dreary desert, covered with shingle; but in its south-eastern portion, the character of the plateau changes entirely. A deep erosion makes of it an alpine mountain region. Wide valleys and deep gorges alternate with stony ridges; the routes and the footpaths go down to a deep level, or lead to great relative and absolute altitudes. Regions of soft and of rough climate, of rich and extremely poor vegetation, rapidly alternate. This alpine character already appears in the basin of the Blue River, but it is still more evident in the basin of the Mekong, where the valleys are still deeper and their vegetation still more varied. Forests of fir and of a tree-like Juniperus make their appearance, as also growths of birch, wild apricots, apple trees and a variety of bushes. In the thickly wooded gorges, the expedition also found the conspicuous white Tibet pheasants (*Crossoptilon thibetanum*), the green *Ithaginis Geoffroyi*, the *Tetraophasis obscurus*, *Tetrastes Sewertzowii*, several species of blackbirds and a good number of the smaller birds of the Passeres group. On a bright clear day the forests and the meadows are full of bird-life. Small colonies of monkeys stay in close proximity to the Tangute encampments.

On June 13 the expedition reached at last the two lakes Orin-nor and Jarin-nor, whence it proceeded to Tsaidam, and then once more across the Gobi, back to Kiakhta. It appeared that everything was in order at the meteorological station, where regular observations were made for a full year. As to the collections made in Tibet, they were very rich and contained no less than 120 mammals, 600 birds, more than 600 species of plants (10,000 specimens) and about 300 specimens of rocks. Latitudes and longitudes were determined in thirteen different spots. The expedition is now back at St. Petersburg. P. K.

CATALYSIS.[1]

The idea and name of catalytic action were introduced into science by Berzelius in 1835, apropos of Mitscherlich's work on the formation of ether. Berzelius pointed out that the action of sulphuric acid in this case was analogous to the action of dilute acids on starch, to the similar action of malt extract, to the decomposition of hydrogen peroxide by metals and oxides, and to the action of platinum on combustible mixtures of gases. According to Berzelius, catalytic force appeared to consist essentially in this, "that substances by their mere presence and not by their affinity have the power to rouse latent affinities, so that compound substances undergo reaction and a greater electrochemical neutralisation occurs." Berzelius made no attempt to explain the phenomenon; on the contrary, in a subsequent discussion with Liebig, he insisted on the great danger of attempting to explain incompletely understood phenomena by hypothetical assumptions, lest experimental investigation should thereby be hindered. Berzelius' warning was not heeded, and the neglect of it is felt to the present day.

Catalytic actions may be divided into four classes:—(1) Release in supersaturated systems. (2) Catalysis in homogeneous mixtures. (3) Heterogeneous catalysis. (4) Enzyme actions.

(1) This first division includes phenomena which may be regarded as fundamentally explained. The best-known case is the crystallisation of a supersaturated solution, for example, of Glauber's salt, by the admission of a very small trace of the solid substance with respect to which the solution is supersaturated. We notice here in the first place the characteristic disproportion between the quantity of the acting substance and the quantity of substance changed by its influence. By a particle of dust far below the limit of what is ponderable, it is possible to bring an indefinitely large quantity of supercooled solution to congelation. The smallest particle which suffices is between 10^{-10} and 10^{-12} gramme. The processes are not limited to supersaturated solutions of solids; they are applicable also to solutions of gas. In these cases a trace of a gas may cause the liberation of an entirely disproportionate amount of another gas. Then again, supersaturation is not limited to the liquid state. Vapours can be supersaturated in respect to liquids and solid bodies, and even in the case of solids, cases are known where they are supersaturated in respect to liquids, that is to say that when they are in contact with a small quantity of liquid in question they are converted into liquid. Supersaturation on the part of solid bodies in respect to the solid bodies which can be produced from them are very common. On the contrary, supersaturation of a liquid in respect to another liquid has not been observed and would be difficult to obtain.

The theory of all these phenomena is known. In all cases we have the formation of a system the stability of which is not the greatest possible under the given conditions of temperature and pressure. There are, on the contrary, other more stable conditions which are characterised by the fact that in them a new phase, that is a physically different component with other properties, makes its appearance. In the case of a supersaturated solution of Glauber's salt, this is the solid salt; in supersaturated soda-water, it is carbonic acid gas. As a rule such a new phase does not appear spontaneously if the supersaturation is not too great, and the system behaves as if it were in equilibrium; but if a small quantity of the absent phase comes in contact with the metastable system, the action is set going and the new phase increases until equilibrium is reached.

If the new phase is a solid substance, the releasing action is associated with a solid nucleus of the same composition. Isomorphous substances have also the property; other solid bodies, on the other hand, are without action. There is here opened a wide field for investigation, since isomorphous substances probably act by the formation of solid solutions, and it is to be ascertained whether solid substances which are not isomorphous with the substance concerned, but are capable of forming solid solutions with it, are active. Further, there are cases where solid bodies act without being isomorphous or without forming solid solutions. Such artificial nuclei can be prepared, for example, by allowing silicic acid to deposit in presence of a crystal and then dissolving the crystal by means of a suitable solvent. This subject has not been fully investigated, but it explains many apparent contradictions that have occurred in the investigation of this difficult subject. Whilst the nuclei in cases of supersaturation in reference to a solid phase must be of a specific nature, in the case of supersaturation with gases any gas whatever will act as a nucleus. This is in consequence of the fact that every gas dissolves without limit in every other gas, that is, forms a homogeneous mixture with it.

A given liquid can be simultaneously supersaturated with respect to different phases, for example, one can easily melt together sodium acetate and sodium thiosulphate to form a liquid, from which, by the addition of a nucleus of either salt, that salt is separated, whilst the other remains dissolved.

Supposing we had such a liquid flowing through a tube at one point of which was a nucleus of acetate and at another of thiosulphate, then each nucleus would continue to grow in its own way as the liquid circulated. We have here an example of the physicochemical possibility of certain organic processes to which Berzelius alluded, such as the formation of the most different substances in the animal body from one and the same liquid, namely the blood. If we might consider the blood as a supersaturated solution in respect to all these substances, it would be intelligible that every organ could increase its substance from one and the same liquid. It would be inadmissible to suppose that we have here a general theory of animal secretion, for the consideration only applies to heterogeneous phases.

Again, there is the question as to whether a compound which does not preexist in the liquid, but can only come into existence by the action of contained substances, is capable of exhibiting supersaturation in regard to other phases in contact with it. There are phenomena of supersaturation known to us in connection with calcium sulphate, solutions of which are so dilute that the salt must be almost entirely in the form of its ions. Since there are no ions in the solid salt there must be here a chemical change. Dilute solutions of lead salts and thiosulphates likewise show a supersaturation in respect to lead sulphide, which is formed from them by complicated chemical decomposition. Finally, the "physical development" in photography affords examples of such phenomena.

Further examples of possible physiological applications cannot be given here, but it is probable that many problems of organic life may find solution by the application of such principles. If we generalise the conditions which have been described, we see that the most essential condition is the presence of a metastable system which only passes to the more stable state by its inherent forces, after the way has been opened to it. The nucleus of the other phase is not the *cause* of the reaction in the sense that Robert Mayer uses this word, because it does not supply the free energy necessary for the process, but is only the means of starting a process which goes to completion by its own forces after it has once been started. We may now be quite clear that the same thing must hold for all other cases of contact action. It is just the want of proportionality between the quantity of the catalytically acting substance and the amount of change which makes it a necessary condition that the changes produced catalytically supply the necessary energy themselves. The recognition of this is apparent with Berzelius when he says that by such a process "a greater electrochemical neutralisation" is effected. One of the most prominent investigators of catalytic enzyme actions states that endothermic actions can never take place in this way. This is quite incorrect, for since endothermic reactions take place of themselves, that is, without catalytic influence, there is no reason for supposing that they should not take place under enzyme influence. It is, indeed, true that under this influence no reactions are possible in which there is a reduction of free (but not of total) energy. In other words, no reactions are possible under the influence of catalysts that could not take place in their absence without a breach of one of the laws of energy. The same is to be said about the view that catalysts can only have a decomposing and not a synthetic action. Organic chemistry affords many examples to the contrary. It is only necessary to refer to syntheses by the catalytic influence of potassium ethylate.

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[1] The phenomena of catalytic action have been the chief subject of investigation by Prof. Ostwald and his pupils during the past few years. An account of the chief results so far obtained, together with a statement of his own views of the general character of catalytic phenomena, was given by Prof. Ostwald in September last to the German Naturforscher-Versammlung at Hamburg. What is here given is a slightly abridged translation of this address. —A. S.

(2) Catalysis in homogeneous mixtures.

We now come to the largest and theoretically most important class of contact actions. The explanation given for the first class cannot here be applied, for while the essential thing in the first class was the appearance of a new phase, this is now excluded by definition. We get the right standpoint for regarding the new problem by adhering to the condition which has just been laid down for all systems that undergo contact action:—the system must not represent a stable condition, for such a system can undergo no change without the addition of energy. How, then, do unstable systems behave when they are homogeneous? The answer is that homogeneous unstable systems cannot exist otherwise than in a state of change. The supersaturated solution can, if the supersaturation is within certain limits, be kept unaltered for an indefinitely long time, when properly protected. A liquid, however, which without the addition of free energy can produce other soluble liquid products cannot be kept without forming these products. This change may proceed, no doubt, very slowly, so slowly that without tedious investigation specially directed to the point no change at all can be observed, but the surest basis for general conclusions that we know—the laws of energetics—demand, as a matter of fact, that the change must take place. They prescribe no numerical value to the velocity; they only demand that the velocity be not strictly zero, but that it shall have a finite value.

By these considerations we gain now for this present case the definition of a catalyst.

A catalyst is any substance which alters the velocity of a chemical reaction without appearing in the final product.

In this definition we purposely avoid expressing any view as to what is the cause of such influence. We must, indeed, take care only to state that for all catalytic actions, causes of the same kind are at work. We seek at present only a definition which will be helpful towards a scientific investigation of the question.

That the definition just given fulfils this purpose will be at once recognised, for it leads directly to inquiry into the numerical value of the acceleration or retardation, and the dependence of this on the nature and concentration of the catalysts, the temperature, the presence of other substances, &c. It is evident, and it must be insisted upon, that all attempts to propound theories for the cause of catalytic phenomena will remain useless until quantitative measurements of the kind referred to have been made.

Regarding catalysis in the sense just defined, it is an extremely common phenomenon, one, as a matter of fact, that appears wherever the velocity of chemical reaction can be measured. The well-known researches of Menschutkin furnish a capital example. He proved that for a number of different kinds of reactions, the velocity varied between very considerable limits, according to the solvent. We must regard this effect of the solvent as catalytic. We do not thereby prejudice the further question as to the actual cause; whether, for example, there is combination between the solvent and the reagents, leading to an alteration of the active masses and so to alteration of the velocity.

We may bridge over the gap between influences of this kind and others in which vanishingly small quantities of an additional substance alter the velocity to a very high degree. Hitherto only the exaggerated cases have been called catalytic, but as the difference is only quantitative a division between them is not justifiable. So far the cases which have been investigated and measured are those in which great influences are exerted by small quantities of material, but restricting ourselves even to these, the number of cases is extraordinarily great. We are especially indebted to the labours of Schönbein for an almost endless list of such reactions. Schönbein certainly did not know that he was dealing only with accelerations of slow spontaneous processes; on the contrary, he looked upon them as being initiated by catalysts. We are thus confronted with the task of subjecting the raw material of this untiring and original investigator to a quantitative revision, a labour which would engage the united activities of a whole series of workers.

I will not attempt to enumerate such cases: I will only remark that there seems to be no kind of chemical reaction which cannot be catalytically influenced, and no chemical substances, whether elements or compounds, which cannot act catalytically. Likewise the answer to the question already asked by Berzelius, as to whether there are general or specific catalysts, must be that both kinds exist. Whilst, for example, the presence of hydrogen ions accelerates most chemical reactions, so that they must be regarded as catalysts of great generality, there are, especially among the enzymes, specific catalysts which only exert their accelerating action on perfectly definite substances. The other question of Berzelius, as to whether different catalysts can produce different products from the same substance or substances and whether different possible reactions in a given system can be effected in different ways by different catalysts, must, I think, be answered in the affirmative, although no special experiments have been made in this direction.

The first theory of catalytic phenomena was set up by Liebig and for the purpose of showing that Berzelius' conception was superfluous. Liebig regarded catalysis as the direct consequence of the law of inertia. His statement is as follows:—"The cause lies in the capacity which a substance undergoing decomposition or combination, that is, in chemical activity, possesses of arousing in a body in contact with it the same chemical activity, or of making the body susceptible of the same kind of change. This capacity is best illustrated by a burning substance, by means of which similar activity is aroused in other bodies when we bring the burning one in contact with them."

Liebig has obviously not been felicitous in this explanation. His example recoils upon him, for in order to ignite the substance we do not require a burning body, but a hot one, and it is a matter of indifference whether it is hot by means of chemical reaction or from any other cause, for example an electric current. Such objections as these were raised to Liebig's view, and he found himself obliged to give his hypothesis a different form. He expressed his view in reference to sugar fermentation in the following words:—"Just as heat is capable of disturbing the statical moment in the elements of many chemical compounds, so this can happen by means of a substance of which the elements are themselves in a condition of disturbed equilibrium; the motion which its atoms possess is imparted to the atoms of the elements of sugar; they cease to persist in that state in which they form sugar and arrange themselves according to their special attractions."

This hypothesis of molecular vibrations has enjoyed grea popularity, and represents even to-day the view of many people, especially of those who have not taken part in the investigations. It has the special advantage that it cannot be disproved, since it is altogether inaccessible to a test. The scientific unfruitfulness that lies in such a "theory" was the less perceived, inasmuch as the development of the rest of chemistry was taking place in a direction where the employment of molecular hypotheses had the value of a very important scientific auxiliary

When, however, we attempt to derive from it the slightest guidance towards experimental and exploratory work, or to deduce an idea of the possible laws of catalytic actions—and this is, indeed, the only purpose of such hypotheses—we are convinced of its entire fruitlessness. That the hypothesis of molecular vibrations drove the whole matter into a blind alley is obvious from the fact that steady scientific investigation of the problem, previously prosecuted with much zeal, did not follow. For quite a long time only isolated investigators concerned themselves with describing catalytic phenomena. Schönbein himself, to whose investigations we are so much indebted for what we know of the facts, took no part in the theoretical discussions as to the causes; on the contrary, it obviously gave him pleasure to investigate phenomena for which contemporary chemistry, to which he paid little heed, could find no explanation or place of refuge.

It is possible to speak much more favourably of another view which had long before been advocated, but meanwhile had been neglected. This is the idea of *intermediate reactions.* This had its origin in the first scientific investigation of the chemical processes that take place in the lead chamber in the manufacture of sulphuric acid. Clement and Desormes, in the year 1806, in a classical research gave the explanation, still generally accepted, of the action of oxides of nitrogen in the oxidation of sulphurous acid by atmospheric oxygen. As everyone knows, this rests upon the assumption that the sulphurous acid is oxidised by higher oxides of nitrogen, which are thereby reduced to nitric oxide. This then unites again with atmospheric oxygen, and the process can begin anew. Thus a very small quantity of oxides of nitrogen serves to oxidise a large quantity of sulphurous acid. It is remarkable that at the time of the discussion between Berzelius and Liebig this case was not brought forward, and only later do we find applications of this old view to other cases where similar chemical processes are brought about. Then, however, this mode of viewing the phenomenon became more and more general, and to-day we must regard it as the oldest and most important attempt to explain certain, though not all, catalytic processes. As a rule, however, there certainly exists a weakness in this view. In confronting a catalytic phenomenon, we seek for the possible intermediate product in the formation of which the catalyst could take part, and we consider the problem essentially solved when we can fix upon such. If we succeed, indeed, in getting some of the assumed intermediate products from the materials, the view is considered to be proved. Whether such a substance is truly an intermediate product, and not merely some bye-product, is a question which is hardly raised and still less answered.

If we test the idea from our present standpoint, we find at first something contradictory in it. In order that a process can occur at all, it must be associated with the diminution of free energy. This loss depends only on the initial and final stage of the reaction, not upon its course. On the other hand, the velocity of the reaction in strictly comparable cases is proportional to this loss. Hence we should be inclined to conclude that the velocity of reaction in a given system must have the same value, whether the process is direct or indirect—whether it takes place in one leap or in several steps.

Such a conclusion would be incorrect, for besides the loss of free energy, the velocity of reaction depends upon many other factors, which are by no means all known. A well-known example is the very great influence which temperature has, an influence which counts much more than the corresponding increase of free energy. Chemical energetics also teach us that while definite generalisations may be made concerning the equilibrium of a given system, this is not the case with the actual velocity with which equilibrium is attained. There is thus no contradiction of general laws, if we assume that a certain course of reaction takes place more quickly through an intermediate substance than it does directly. Nothing can be said for, but something against, the view that this is generally the case.

Coming back now to our classical example, which indeed in other respects will be historical, we can assume that sulphurous acid is oxidised more slowly by the oxygen of the air than by the two reactions. Oxidation of sulphurous acid by nitrogen peroxide and oxidation of nitric oxide by atmospheric oxygen run concurrently, although the concentration of the intermediate products must necessarily be smaller than the concentrations which act in a direct reaction. Whilst, however, we may regard this view as scientifically founded, there is still a chief thing wanted. The velocity of the reaction concerned must be actually measured, and until this is done one can only speak of a conjecture and not of an explanation, and what is said here is true in general for the assumption of an intermediate reaction. No catalytic acceleration is explained, unless it is also shown that the intermediate reactions actually take place more rapidly than the direct reactions, under the given conditions. Up to now, no case of this kind has been satisfactorily investigated, and no such explanation actually proved for a given case. I certainly hope that this gap will not long remain, as investigations directed to this point are approaching their conclusion.

If we assume that in certain cases the correctness of the theory of intermediate products is proved (which to all appearances is the case), we have the further question, whether all catalyses will find an explanation in this way. I believe this question may certainly be answered in the negative. I believe that there are a number of cases in which such an explanation is not applicable. In particular, I see no possibility of explaining the retarding catalytic influences by the assumption of intermediate products, for if a reaction goes more slowly *via* the intermediate products than in the direct path it will take the latter, and the possibility of intermediate products has no influence on the process.

Another theory of catalysis has been put forward by Euler. Starting from the assumption that all chemical reactions are reactions of ions and that the velocity depends on the concentration of active ions, he supposes that the catalytic substance has the property of altering the concentration of the ions. In accordance with this alteration in the concentration, the velocity of the reaction must also alter.

So far as I can see, such a theory is admissible, that is to say, it would usually be possible to make the assumption without contradicting the laws of general chemistry. Whether or not ultimately contradictions would appear, cannot be foreseen at present. There appears to be one difficulty in the fact, often observed, that two catalysts acting together effect a much greater acceleration than would be calculated from the sum of their separate effects. It is not obvious how, by the simultaneous action of the two catalysts (for example, Cupriion and Ferriion), so much larger quantities of reactive ions could be formed than these can form acting apart.

One may say of this theory, then, that it might explain some catalyses, but by no means all.

A more complicated case of catalytic phenomena is found in processes where one of the substances taking part in the reaction acts also in the capacity of catalyst. Among the possibilities of this *auto-catalysis*, I will only mention the case of a reaction producing an accelerator. This occurs, for example, in one of the best-known reactions, the solution of metals in nitric acid. The nitrous acid here formed increases in a high degree the velocity of reaction of the nitric acid, whence arise the following phenomena:—If the metal is brought into pure acid, the reaction begins with extreme slowness. In the same degree as it progresses, it becomes more rapid and in the end tumultuous. When this period is passed, the process slows down and ends with a velocity converging on zero.

This stands in striking contradiction to the usual course of reactions, which begin with a maximum of velocity and, owing to the gradual consumption of active substances, become constantly slower.

Physiological analogies present themselves irresistibly at this point. We have here a typical *fever* phenomenon. Another important physiological fact can be illustrated in the same way, *habit* and *memory.* I have here two specimens of the same nitric acid, differing only in this, that in one a small piece of copper has been already dissolved. I now bring two similar pieces of copper foil into the two acids, which are at the same temperature. You see that the acid which has already once dissolved the copper has become "habituated" to the work and begins instantly and vigorously to carry it out, while the other unpractised acid does not know how to begin with the copper, and sets about its work so clumsily and so slowly that we will not wait for it.

That we are here dealing with catalysis by means of nitrous acid will be obvious when I add some sodium nitrite to the dilatory acid; the copper is now at once attacked and dissolved.

(3) Heterogeneous Catalysis.

The best-known case of heterogeneous catalysis is the action of platinum on combustible mixtures of gases. Whilst previously the chief interest centred round mixtures of hydrogen and oxygen, it has now passed for practical reasons to the combustion of sulphur dioxide to trioxide. In this case we are concerned, no doubt, with slow reactions, although it may be admitted that, for example, in the case of electrolytic gas, no formation of water is perceptible at the ordinary temperature in the absence of a catalyst. But the regularity of the alteration of the velocity with the temperature justifies us in the opinion that after all there is a very small velocity of reaction even at the ordinary temperature. The extremely small value is in accordance with the general fact that a gas reaction takes place very slowly. This important fact appears, for example, in the experiments of Berthelot and Péan de St. Gilles. The ester formation from acid and alcohol was compared in two experiments of the same kind, in one case the substances being liquid, in the other gaseous. Even if the experiment does not permit of an exact calculation as to whether the retardation may be fully explained by the great reduction of concentration, or whether, as is more probable, it goes beyond that, it is not to the point. It is sufficient to know that by transition to the state of gas, the velocity of the reaction is reduced to about 1/1000th.

It is possible now to set up a theory of the accelerations just mentioned. If we suppose that in the gaseous system, at a given temperature, one part is replaced by a liquid or acquires a density corresponding to the liquid state, then in this part the reaction will take place proportionately more quickly, and the liquid part of the materials will be converted into the final products. If then the liquefying or condensing source is of such a nature that it goes on condensing fresh quantities of materials as fast as the old are being used, these will react quickly and the result is acceleration of the reaction. That such is the case in the action of platinum on gases is quite possible. I do not wish to assert by this representation that the catalytic action takes place in such a way, but only to point out a possibility as to how it might take place. We should then have the simplest and purest case of an accelerating intermediate reaction to which I have already referred.

As Prof. Bredig has recently told me, it is possible to represent the mechanism of such an acceleration by means of a fluid medium in which small masses of another fluid are suspended. If this suspended liquid has the property that in it the reaction of the substances present can take place more quickly than in the main mass of the liquid, the portions of the reagents residing there will be the first to undergo change. The products will diffuse into the enveloping liquid, and thus new quantities of reagents will enter, since diffusion is always at work regulating the concentration. In this way the whole quantity of the reagents will successively find a way through the suspended liquid and react there, and the result is an acceleration of the reaction.

Bredig supposes that this view may be applied to the case where a catalyst is present in a colloidal state in the liquid. As is known, Prof. Bredig and his pupils in a series of remarkable investigations have demonstrated the manifold and energetic catalytic actions which are brought about by colloidal platinum and other colloidal metals prepared by him. He also pointed out that the extremely active catalysts occurring in nature, the enzymes, occur likewise in a state of colloidal solution or suspension.

These views have no other pretension than to be views which can be brought to an experimental test. I must not omit to draw attention to the fact that it is the view of catalysts as accelerators that has made it at all possible to put forward notions which can be so tested. Let anyone try to attain the same end by means of molecular vibrations.

(4) Enzymes.

Berzelius had no doubt that the conversion of starch into sugar by means of acids was to be classed with similar conversions by means of malt extract. The same view was held by Payen and Persoz, who isolated the active substance, diastase, or at least prepared it in a concentrated form. The same view holds good for Liebig and Wöhler, who in an excellent research studied the decomposition of amygdalin under the catalytic influence of emulsin.

The later investigations of the laws of enzyme action have, in my opinion, brought to light nothing which gives ground for a fundamental distinction between the two kinds of action. On the contrary, the researches of Bredig, before mentioned, have displayed a much more thorough-going correspondence than might have been expected.

We shall look upon the enzymes, therefore, as catalysts which arise in the organs during the life of the cell and by whose action it discharges the greatest part of its duties. Not only are digestion and assimilation from beginning to end regulated by enzymes, but the fundamental life activity of most organisms, that is to say, the acquisition of the necessary chemical energy by combustion in atmospheric oxygen, takes place with the definite cooperation of enzymes, and without this would be impossible, for free oxygen is known to be a very inert substance at the temperature of organisms, and without an acceleration of the reaction, the maintenance of life would be impossible.

Berzelius had already pointed to the decisive importance that attaches to enzymes in the economy of the living being. As a matter of fact, if we put the fundamental question, "what is the physicochemical criterion of the phenomena of life?" the answer will be, "an automatically regulated production and use of chemical energy, for the animation, maintenance and increase of the living thing." We have three different means of influencing the velocity of chemical reaction—temperature, concentration and catalysis. Of these three, the first cannot be obtained for the organism at its will. We see, indeed, that in the higher animals, in which especially complicated and delicate functions are to be fulfilled, there is complete freedom from the influence of temperature, inasmuch as they provide themselves with thermostatic appliances by means of which their body temperature can be maintained constant between certain limits. Concentrations are in many ways limited by the solubility of the substances concerned. There remains only one generally applicable means of regulating the velocity of the reactions—the application of catalysts—and this without doubt admits of the solution of the problem in ideal completeness.

I must not go further into the physiological question, but I did not wish to omit pointing out the importance of catalysis in this direction. This seems to me to be at the present time especially necessary. The older chemistry, with its facts and theories, which were concerned with the preparation and the systematic and genetic relations of substances, without regard to the laws of equilibrium and change, has proved in many respects unproductive in the explanation of physiological phenomena, and it seemed as if chemistry and physics were altogether unable to contribute anything decisive towards solving the riddle of life.

On the other hand, I should like to point out with all reserve that physical and general chemistry, in the domain of which the greater portion of this question lies, is a very young science. Those of you who were present at the Naturforscher Versammlung in 1892 will remember that it first, so to speak, "came out" there like a full-grown young lady. Hitherto she has found so much to do in her own house that she has only been able occasionally to pursue her labours in other regions, but it cannot be denied that many a hand has wished to pluck the fruits without knowing what to do with them.

It is my full and repeatedly expressed opinion that by means of the later advances of chemistry there lies before physiology a development which will be in no degree less important than that which was brought about by Liebig in his time by the first systematic application of chemical science.

So far as the properties of the enzymes are concerned, these have hitherto been investigated mainly in a qualitative way. Quantitative work meets with the great difficulties which lie in the alterability of the substance, associated, as a rule, with the loss of catalytic power. The enzymes hitherto investigated show essentially the chemical properties of albumenoids, but the question of their chemical nature is by no means yet settled. I should like to express my conviction that a more thorough investigation will disclose other intermediate stages between albumenoid products with which enzyme activities have been shown to be associated and the more simple compound substances of organic chemistry. Thus, for example, the catalytic acceleration of some oxidation processes which are characteristic of hæmoglobin seems to be maintained in its non-albumenoid derivatives, especially in hæmatin, and a following out of these relations in the decomposition products of the colouring matter of the blood would be of no slight interest.

In a few cases in which the cause of the velocity of an enzyme action has been studied in such a way as to be completely free from objection, contradictory results have appeared; while some authors have found a thorough-going agreement with the simple laws which are applicable to inorganic catalysts, others have found discrepancies.

From a hitherto unpublished article which lies before me, and in which I am inclined to place great confidence, I gather as a matter of fact that the time law of enzyme actions differs from the simplest scheme of reaction velocities. This question is, however, not yet ripe for discussion.

The question of the range of substances which can be altered by a given enzyme in a certain manner (for example, hydrolysed) is likewise in the first stage of solution, and there seems to be here similar multiplicity of function to what is found in the case of other catalysts.

The beautiful investigations of E. Fischer have shown that at any rate the very slight differences which nowadays we know in chemistry as stereochemical can bring about alteration in the action of a given enzyme. As to whether this rests on the asymmetric character of the enzyme itself or on other grounds appears to me not to have been decisively ascertained.

I must hasten to a conclusion. I have set myself the task of pointing out the broad provinces of a fertile land, which only here and there shows the first beginning of systematic cultivation, but of which the fruitfulness and importance is beyond all question. Even if the land lies outside the region to which the chemistry of the past was acclimatised, still our restless science has already begun with its new implements to make the new soil fruitful. That it is not only a chemical interest that makes the work grateful I think I have shown you by examples of its physiological application. It is also evident, from the examples which we already have of the application of this auxiliary, that the scientific knowledge and investigation of catalysis must have vast consequences in technical applications. The last great triumph of German technical chemistry, the synthesis of indigo, which will revolutionise the agricultural conditions of whole countries, contains as an essential factor a new catalysis. The oxidation of naphthalene by means of sulphuric acid with speed can only be brought about in the presence of mercury. The sulphuric acid itself, it is hardly necessary to say, is prepared by a catalytic process, whether we use the old or the newer method. When we consider that the acceleration of the reaction by catalysis is achieved without consumption of energy, and so proceeds in this sense *gratis*, and that in chemical industry, as in all other, time is money, we perceive that the systematic utilisation of catalytic appliances is likely to lead to the most thorough-going changes in manufacturing processes.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

THE Government Education Bill has been the subject of much discussion since Mr. Balfour described its provisions to the House of Commons last week. Public opinion is decidedly in favour of the creation of local educational authorities, but it is felt that unless these new bodies are made responsible for elementary as well as secondary education, the main object of the Bill will be lost. As the president of the National Union of Teachers pointed out at the conference at Bristol, there would still exist in the same district "separate authorities for primary and secondary education, with their useless and unnecessary administrative expenditure, their jealous rivalries and interminable friction." The only way to end this state of things is to make each constituted local authority responsible for the whole of the educational work in its district. There must be no clause making it optional to adopt the elementary part of the measure, for in many cases this would mean that there would still be competing schools and educational agencies instead of an organised system. The members of School Boards who have a real knowledge of education would naturally be absorbed by the local authorities, and those who are more identified with sectarian and political interests would be left to find another platform for their polemics. The views of teachers in primary schools are expressed in the following resolution brought before the Bristol conference by Mr. R. Waddington:—"That conference expresses approval of the main principles of the Education Bill, 1902, under which may be created local authorities controlling and maintaining all forms of education within wide areas, and hereby records its satisfaction with the Government's desire to place our educational system on a sound basis; but is of opinion that the measure cannot become educationally effective unless the permissive clauses of the Bill relating to elementary education be struck out, and it be made compulsory upon the local authorities to take over the control of elementary as well as of higher education." If the Government decide to withdraw the optional clause the measure will meet with general approval from most educationists.

PROF. R. MELDOLA, F.R.S., has been appointed by the President of the Board of Education a member of the Teachers' Registration Council, which has just been created to consider claims to be admitted to the Register of Teachers.

THE Lord Mayor of Liverpool has issued an appeal for funds to establish a Liverpool University upon the University College of the city. To effect this, about 330,000*l*. will be required, of which there has already been promised no less than 145,000*l*. by leading citizens. The present value of possessions of the College itself amount to more than 500,000*l*.; and the additional sum of 330,000*l*. which is asked for is to complete its equipment as a university. The existing resources of the College, the endowment of chairs and lectureships, amount to 186,300*l*.; the sites acquired and buildings erected and in course of erection, 251,550*l*.: fellowships, scholarships and prizes, 32,800*l*., exclusive of value of fellowships and scholarships established by annual gifts or granted by city and county councils, the Royal Institution, the Ladies' Educational Association, the Tate trustees, and other bodies outside University College; endowments for maintenance, 20,275*l*.; and day training college hostel and endowment, 10,000*l*. The total of 500,925*l*. does not include the value of books in the library and apparatus in laboratories, nor does it take account of sums, amounting to many thousands of pounds, given to the college year by year for immediate expenditure, nor of the annual income of the affiliated schools of architecture and applied art, public health and tropical medicine. The additional lectureships to be endowed include electrotechnics, geology and chemistry, besides others in connection with commerce, engineering and medicine.

SCIENTIFIC SERIAL.

American Journal of Science, March.—The ventral integument of trilobites, by C. E. Beecher. In previous studies of trilobites the author had not thought it worth while to illustrate the character of the ventral integument, but a recent discovery by Jaekel necessitates the separate consideration of this structure. From a study of a specimen of *Ptychoparia striata*, Jaekel has deduced an entire reconstruction of the appendages and anatomy of the trilobite. An examination of well-preserved specimens of Triarthrus, several photographic reproductions of which accompany the paper, leads to the conclusion that the deductions of Jaekel are erroneous.—Igneous rocks from eastern Siberia, by Henry S. Washington. The specimens examined included a foyaite from East Cape, comendite, quartz-porphyry, rhyolite, obsidian and monzonite from Iskagan Bay.—A cosmic cycle, by Frank W. Very.—Studies of Eocene mammalia in the Marsh collection, Peabody Museum, by J. L. Wortman. The present instalment is devoted to a consideration of *Limnocyon verus*, *velox*, *medius* and *dysodus*.—An experimental method in the flow of solids and its application to the compression of a cube of plastic material, by J. R. Benton. Frames of parallel wires were cast into the centre of a cube of Wood's metal. After the cube had been distorted beyond the elastic limits in a testing machine, the fusible metal was melted off and the structure of the framework examined. The condition of the wires after varying treatment is shown in a series of diagrams.—On the occurrence of monazite in iron ore and in graphite, by O. A. Derby.—The molecular weights of some carbon compounds in concentrated solutions with carbon compounds as solvents, by C. L. Speyers.—Clarence King, by S. F. Emmons. An account of the life-work of the late Clarence King.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, March 6.—"Experimental Researches on Drawn Steel.—Part i. Magnetism and its Changes with Temperature.—Part ii. Resistivity, Elasticity and Density, and the Temperature Coefficients of Resistivity and Elasticity." By J. Reginald Ashworth. Communicated by Prof. Schuster, F.R.S.

When magnets are heated and cooled and the cyclic state is reached, the relation of intensity to temperature is expressed by the equation

$$I_t = I_0 (1 + a t),$$